food webs, farm food webs, forestry food webs, orchard food webs or motorway embankment food webs, despite the fact that these habitats can be extensive and are much more intimately connected to humans than natural habitats."

"A major problem lies in the extent to which constituent species are often 'lumped' into functional groups — each bird lovingly identified, while individual arachnid species become one unitary 'spiders' in ways which can bias or cloud analyses," says May.

"Our general quest for understanding how the world works, and what is our place in it, is the quintessential characteristic that sets humanity apart from other animals. But better understanding of ecosystem assembly and collapse is arguably of unparalleled practical importance for ourselves and other living things, as humanity's unsustainable impacts on the planet continue to increase."

Dobson and his colleagues make a more general plea for food web theory to be widely considered in plans for the management of national parks and the biodiversity they seek to preserve. "A considerable urgency drives attempts to assemble data from large undisturbed and pristine ecosystems such as tropical grasslands, forests and coral reefs." If the principal arguments for conserving natural ecosystems are based purely on economic benefits then we need to develop a theory that links ecosystem services to food web structure, they say.

"Our general quest for understanding how the world works, and what is our place in it, is the quintessential characteristic that sets humanity apart from other animals. But better understanding of ecosystem assembly and collapse is arguably of unparalleled practical importance for ourselves and other living things, as humanity's unsustainable impacts on the planet continue to increase," says May.

Q & A

Edward A. Kravitz

Ed Kravitz is the George Packer Berry Professor of Neurobiology at Harvard Medical School and is a graduate of the City College of New York (BS degree) and The University of Michigan (Ph.D. in Biological Chemistry). From post-doctoral studies at NIH, he went to Harvard Medical School in 1961, where he became professor in 1969 and where he still is an active faculty member. His research interests have centered on neurotransmitters and neuromodulators, and now focus on the roles of such substances in aggression using the fruit fly, Drosophila melanogaster*, as a model organism. In early studies, Ed and his colleagues (Steve Kuffler, Dave Potter, Masanori Otsuka, Les Iversen and Zach Hall) were the first to demonstrate that GABA is a neurotransmitter; with Tony Stretton, Kravitz was the first to demonstrate that an intracellular fluorescent dye can be used to determine neuronal geometry. A member of many scientific and honorary societies, Ed Kravitz is most proud of his 'Lifetime Achievement in Mentoring' award from Harvard Medical School, the 'Education' Award from the Association of Neuroscience Departments and Programs, and the Harold Amos Diversity Award for 2007.*

What turned you on to biology in the first place? I always was interested in science without actually knowing much about it. I grew up in the North East Bronx and attended ordinary city schools, where being smart was not necessarily appreciated. I graduated high school barely making the school 'honour roll' acquiring about a 'B' average, and did not do much better at the City College of New York, where mostly I was interested in girls and basketball. I did get a few 'A' grades. One was in basketball and another in Physical Chemistry, the hardest course at CCNY, and the only one I found interesting. My son once looked at my college academic record and said "Dad, I am amazed that you ever got anyplace in the world".

It was my work at Sloan Kettering Hospital as a technician for George Tarnowski, immediately after graduating college, that really turned me on to a career in biochemistry. There, I worked in a cancer chemotherapy laboratory injecting mice with compound after compound in an attempt to stop the growth of implanted solid tumours. It wasn't this work that turned me on though; that came via a different route. A small laboratory adjoined the one I worked in, where a young investigator (Louis Kaplan) was examining the biochemical properties of Ehrlich Ascites tumour cells. Lou was the shortstop on the Sloan-Kettering softball team and he encouraged me to join the team, where I ended up as the third baseman. Through our association, I started asking Lou about his research and as a result got interested in the possibility that I too might start doing 'research' on the biochemistry of tumour cells. Tarnowski said that would be fine after my chores were completed and the head of the tumour biology section agreed, so I started doing 'research'.

Once started, I was hooked. What excitement! That led to an evening course in biochemistry at CCNY and applications for graduate study at Rutgers University and the University of Michigan, the alma maters of scientists in my section at Sloan Kettering. Both accepted me; I chose the latter, ending up in a very old-fashioned biochemistry department that was in transition.

Do you have a favourite paper? In my desk drawer at home, I used to keep a copy of Alexander Fleming's 1929 paper reporting the discovery of penicillin: 'On the antibacterial action of cultures of a penicillium, with special reference to their use in the isolation of *B. influenzæ*' (Br. J. Exp. Pathol. *10*, 226–36). What I most liked about the paper was that something that had surely been seen and tossed away innumerable times before — a mould-contaminated bacterial culture plate — yielded an immense find to an observant investigator, a discovery that has saved countless lives worldwide. What Fleming did was pay attention to a bacteria-free clear area surrounding the mould. In essence, he followed the timeless advice of Yogi Berra, the former catcher and manager of the New York Yankees: "you can observe a lot by just watching". While Fleming apparently wasn't a good enough chemist to complete the purification of 'penicillin' (a name he coined for a clear sterile filtrate of the broth of

a mould culture), he did explore its chemical properties, showed that it was far better than any existing antibacterial agent, demonstrated that it was relatively non-toxic, and possibly most dramatically, showed that it could be topically and effectively applied to bandages.

What is the best advice you've been given, and what advice would you offer? Two things come to mind. One is the sage advice of my mentor, Steve Kuffler, who passed away at the young age of 67 in October of 1980. Steve always said "the good old days are now". The message is clear. Do not live in the past, live in the present and enjoy and take full advantage of the opportunities now available to you. To someone just starting in Biology, my advice always is "in making important career decisions do not rely solely on analysis and logic". The heart should play as important a part in your decision making as the head. Is this truly a place you want to be? Are you selecting your next position based on the ranking of individuals or institutions, or is the choice you are making truly where you want to be?

When I first visited Harvard Medical School to look at a possible academic position there, my biochemical colleagues gave me a list of 'criteria' that should be satisfied before I should consider accepting any position. Steve Kuffler and his group — Dave Hubel, Torsten Wiesel, Ed Furshpan and Dave Potter — had just moved to HMS from Johns Hopkins to start a Neurophysiology Laboratory in the Department of Pharmacology. My biochemist colleagues had never heard of any of these people and I was warned it would end my career as a biochemist if I ended up in a Pharmacology Department. Their list included a position in Biochemistry (not Pharmacology), so much start up money, so much space, an assistant professorship, and so on. Before the visit to HMS, I looked up the publications of members of the group and did not understand anything I read. Instead there were squiggly lines, incomprehensible abbreviations (EPSPs, IPSPs..), and in Steve's papers, cartoons of Steve sticking electrodes into cat brains. I was sure I was wasting my time in this, my first 'job' interview.

There was little communication during my chats with members of the laboratory either — I distinctly remember Hubel and Wiesel glancing sideways at each other and shuddering when I said I was interested in the biochemistry of memory. Then came the time with Steve. He dismissed everything on my list. Instead he offered me five years of research support in a shared research space in Pharmacology, and the opportunity to see if I was any good in science. As I headed back to Washington on the train from Boston, I had an important decision to make: would I accept this offer? Logic dictated 'no'. But here were people who really seemed to know things about the nervous system, and here was an opportunity to see if I could make it on my own as a scientist unhindered by the need to find funding for my research and involved in only limited teaching to medical students. I believe I made the correct choice.

What has been your biggest mistake in research? I wouldn't necessarily call it a mistake, but an early experiment I carried out never was submitted for publication. As a beginning graduate student I became interested in DNA biosynthesis (still a mystery in the mid-1950s). One of the experiments I attempted aimed to identify intermediates in DNA biosynthesis by adding tracer amounts of radioactive guanine to logarithmically growing *Escherichia coli*. Samples were withdrawn at regular times after addition of the radioactive material and, after centrifugation and acid extraction of the cells, these were separated using standard methods of the day into acid soluble, RNA and DNA fractions. Not surprisingly, what I found was radioactivity appearing first in the acid soluble, then in the RNA and finally in the DNA pools. What surprised me, though, was that the counts in the RNA fraction rose to a peak and then were reduced by about 10% after which they remained relatively constant. In the literature of the day, the gospel was that once synthesized, radioactivity incorporated into RNA remained stable. The advice I received from my advisors was, therefore, there is an error in your protocol — don't publish! So I didn't.

Two years later, Astrachan and Volkin did a similar experiment with bacteriophage-infected *E. coli* and discovered what they called 'DNA-like RNA'. They also did not quite recognize what they had in their hands, and never did get as much recognition as they deserved for the discovery of what turned out to be messenger RNA. Still, had I published, it probably would have been a nice first paper for a graduate student. On the other hand, I might not have gone on with my career in neuroscience had I entered the world of the molecular biologists with my first publication, and that truly would have been a mistake and a great shame. Mostly I tell this story when I advise students not to always listen to their advisors — instead, stick to your guns if you feel you truly can defend your territory.

Do you have a scientific hero? Yes, Seymour Benzer, who died last year at the age of 86. Seymour was a wonderful person and a superb scientist. I first met Seymour when he was a 'student' in the summer Training Program in Neurophysiology at the Marine Biological Laboratory in Woods Hole that was nominally run by Steve Kuffler, but was organized and taught by Ed Furshpan and Dave Potter. I took an immediate liking to Seymour and watched in wonder as he wrote, in a miniature script in small notebooks, notes of any and all possible facts extracted from the days conversations. Seymour had a way of asking the penetrating questions that went to the heart of all issues and seemed willing to listen to and learn from all comers.

I remember that on one visit to our department, I told Seymour that lobsters transfer sperm to females during mating via spermatophores (small sacks containing sperm) that the females could hold for up to a year before using them to fertilize their eggs. I also mentioned that these could be released from male lobsters via electrical stimulation. I have no idea why we were discussing this topic, but Seymour insisted that we go to the laboratory that instant to attempt to release a spermatophore from a male lobster. I actually had never done this before but, with an AC power supply and a rheostat, we turned the voltage higher and higher and to Seymour's and my delight, out popped a spermatophore. To my surprise, he immediately asked for a microscope slide and coverslip and rushed us to a nearby microscope to tear open the spermatophore to release and examine the sperm (what else would any reasonable person do...).

What is your greatest research ambition? I want to 'watch' behaviour happening in the nervous system of an intact or semi-intact fruit fly. This will require instrumentation that will allow us to record optically from brain neurons with previously defined functions. We will soon have available many of the cell lines needed to perform these experiments. Many technical hurdles remain before the experiments actually can be carried out — not the least of which is obtaining access to the required two-photon imaging system — but it still amazes me that, by switching to fruit flies, we now can actually think about performing experiments that were only dreamed of such a short time ago.

What do you think are the big questions to be answered next in your field? Behaviour happens in stereotypical patterns which switch from one to the other along statistically determinable lines. Even though we can recognize and score these patterns and they are recognizable and meaningful to other members of the species, each time an animal shows a pattern of behaviour, it is different. We know and continue to learn a great deal about how the 'wiring diagrams' of nervous systems are assembled — clearly transcriptional codes are key elements in that assembly. But the wiring diagrams only represent the scaffolding on which behaviour is constructed. Much less is known about how the scaffolding is assembled into simple pattern generating circuits, although elegant work from Michael Bate's laboratory at the University of Cambridge is beginning to make major contributions in that arena. Still less is known about how these simple patterns are assembled into the complex behavioural patterns noted by observers of animal behaviour. I anticipate that the next decades will see major advances in this arena too, as investigators begin to address questions of how behaviour gets wired into nervous systems — what are the roles of genes? hormones? experience? Once the patterning machinery is established, a next key step will be, how does behaviour work and how is it molded by experience.

Department of Neurobiology, Harvard Medical School, Boston, MA 02115, USA.
E-mail: edward_kravitz@hms.harvard.edu

Slippery issue

This spring, the member countries of the Convention on International Trade in Endangered Species (Cites) have started to regulate trade in the European eel, a highly prized delicacy in Europe and Asia, given the plummeting numbers of this species.

It is widely distributed in coastal areas and freshwater ecosystems in Europe and the Mediterranean and was once abundant. It has one of the most complex life histories of any vertebrate: adults migrate from Europe to the Sargasso Sea near the Caribbean where they breed and then die. The offspring then rely on ocean currents to carry them back to Europe where they enter the freshwater river systems to grow before setting back out for the Sargasso Sea.

"Eels are no longer the familiar sight in European and Caribbean waters that they once were," says Willem Wijnstekers, secretary-general of Cites. "Many people derive a substantial living from fishing for them, while others are involved in the aquaculture which grows the fish to marketable size. All this is under threat unless fishing for European eels is put on a more sustainable footing."

Older eels are caught for their meat but glass eels, an early stage of eel newly returning to Europe, are mostly harvested live to be grown to marketable size in aquaculture facilities predominantly in Asia, but also in Europe. About half the eels caught in Europe are exported to China, Japan and South Korea for this purpose and, in recent years, this amounted to more than 200 million eels per year, Cites says.

Unofficial estimates suggest that, as recently as the 1990s, around 30,000 tons of eels were captured every year, with a value of around 200 million euros and that around

Steep decline: Numbers of European eels are falling dramatically. (Photo: Alamy/David Hosking.)